ROBERT HOOKE AS A PRECURSOR OF NEWTON.

MACH[1] has emphasized that in estimating the achievements of Newton in the theory of gravitation we should not underrate that of the imagination. "We have, indeed," says Mach, "no hesitation in saying that this last is the most important of all." In the present article, I wish to point out the fact that the way for this feat of imagination was amply prepared, notably by Robert Hooke, whose works can hardly have been quite unknown—directly or indirectly— to Newton.

The ideas that the key to the motions of the sun and planets was a gravitational force and that the orbits could be explained by a force deflecting the planets from the straight line which, according to Galileo's law of inertia, they would otherwise describe uniformly, were quite familiar to Newton's elder contemporaries Wren, Hooke and Halley. What made it impossible for them, and possible for Newton, to unlock the door which guarded one of the secrets of the heavens was simply due to the happy circumstances that Newton was a great and far-seeing mathematician, and the time was ripe for the reduction of infinitesimal ideas into a powerful method. The "method of fluxions" was, we know, discovered by Newton even before the falling apple, according to the well-known story, first turned his thoughts toward gravitation.

Newton's feat of imagination consisted[2] in the widen-

[1] *Mechanics,* p. 189.
[2] *Ibid.,* p. 190.

ing of his sphere of thought so as to connect the falling of the moon in her orbit towards the earth with the descent of a stone near the surface of the earth. Newton[3] himself illustrated the identity of terrestrial gravity with the universal gravitation which determines the motions of the celestial bodies by imagining stones to be projected from the top of a high mountain with greater and greater horizontal velocities. Finally the stones become satellites circulating round the earth. Later, commenting on Rosenberger's[4] statement that the idea of universal gravitation did not originate with Newton, Mach[5] says: "But it may be safely asserted that it was with all of them a question of conjecture, of a groping and imperfect grasp of the problem, and that no one before Newton grappled with the notion so comprehensively and energetically; so that above and beyond the great mathematical problem, which Rosenberger concedes, there still remains to Newton the credit of a colossal feat of the imagination." Mach[6] then gave a short account of some of Hooke's researches, and concluded: "Thus Hooke really approached nearest to Newton's conception, though he never completely reached the latter's altitude of view."

It seems to me that we must lay less stress on Newton's feat of imagination and more stress on his great advance in mathematics and its applications to physics than Mach, apparently, would admit. Further, and this seems to me the most important thing from a philosophical point of view, we shall see that Hooke had begun to grasp that principle of mechanics which Newton grasped more firmly in his third "Law of Motion."

In quotations from old works, I shall, since my object

[3] *Ibid.*, pp. 533-534.

[4] *Isaac Newton und seine physikalischen Principien*, Leipsic, 1895, pp. 135-157.

[5] *Mechanics*, p. 531.

[6] *Ibid.*, p. 532.

is scientific and not merely literary, modernize spelling and punctuation. However I shall always quote literally for purposes of reference the *titles* of books and such passages as seem to have a literary interest.

I.

Robert Hooke was born at Freshwater in the Isle of Wight on July 18, 1635. He was very infirm and weakly and was therefore nursed at home, though his brothers and sisters were nursed abroad, and for at least seven years his parents had very little hope of his life, since he was often ill. For his age he was very sprightly and active in running, leaping, and so on, though very weak as to any robust exercise. He was very quick to learn anything, and, after his English, soon learned his Grammar by heart but, as he says, with little understanding, till his father, who was minister of the parish of Freshwater, designing him for the ministry, took some pains to instruct him. But since he was very often subject to the headaches which hindered his learning, his father laid aside all thoughts of breeding him a scholar, and finding that he himself grew very infirm through age and sickness, he wholly neglected his son's further education. Thus Hooke being left to himself spent his time in making little mechanical toys about which he was very intent. He endeavored to imitate everything he saw done by any mechanic; he had also a great fancy for drawing. How he spent the next six or seven years of his life is not known. After his father's death in 1648, he was placed in the workshop of Sir Peter Lely the painter, but presumably stayed there only a short time as the smell of the oil colors did not agree with his constitution. After this he lived with Dr. Busby, master of Westminster School, with whom his education progressed with surprising rapidity both in its classical and mathematical branches. Here he applied himself to Latin

and Greek and at the same time made some acquaintance with Hebrew and other Oriental languages. Here too he began a serious study of mathematics. From Westminster School he went in 1653 to Christ Church, Oxford, as "servitor" (one who performed certain menial duties instead of paying fees) and ten years later he took his M.A. degree by special recommendation of Lord Clarendon, then Chancellor of the university. After 1655 he was employed and patronized by the famous experimental physicist (the Hon.) Robert Boyle,[7] who turned Hooke's skill to account in the construction of his celebrated air-pump. Hooke's inventive faculty exercised itself between 1657 and 1659 in devising thirty different methods of flying, and also—more profitably—in regulating the movements of watches by the application of the balance spring. In 1675 a lively controversy arose between him and Huygens respecting their rival claims to this ingenious invention. The truth seems to be that the original idea belonged to Hooke, but that the coiled form of the spring, on which its practical utility depends, was due to Huygens. On November 12, 1662, Hooke was appointed first curator of Experiments to the Royal Society,[8] and filled the office with extraordinary diligence and skill during the remainder of his life. In 1664 Sir John Cutler instituted for Hooke's benefit a mechanical lectureship of £50 a year, and in the following year he was nominated Professor of Geometry in Gresham College, London, where he subsequently resided. After the great fire in 1666 he constructed a model for the rebuilding of the city, which was highly approved although the design of Wren was preferred. Neither plan was, however, adopted. During the progress of the works Hooke acted as surveyor, and accumulated in that lucrative employment a sum of several thousand pounds. This hoard was dis-

[7] See Mach, *Mechanics*, pp. 123-127.

[8] This society was founded in 1662. Cf. Rosenberger, *op. cit.*, pp. 69-71.

covered after his death in an old iron chest which had evidently lain unopened for about thirty years. He fulfilled the duties of secretary to the Royal Society during five years after the death of Henry Oldenburg in 1677.

A protracted controversy with Hevelius, in which Hooke urged the advantages of telescopic over plain sights, brought him little but discredit. His reasons were good but his offensive style of argument rendered them unpalatable and himself unpopular. Many circumstances concurred to embitter the latter years of his life. The death in 1687 of his niece, Mrs. Grace Hooke, who had lived with him for many years, caused him deep affliction; a lawsuit with Sir John Cutler about his salary—a suit which was, however, decided in his favor in 1696—occasioned him prolonged anxiety; and the repeated anticipation of his discoveries inspired him with a morbid jealousy. Marks of public respect were not wanting to him. A degree of M.D. was conferred on him at Doctor's Commons, December 7, 1691, and in 1696 the Royal Society made him a grant to enable him to complete his scientific inventions. While engaged on this task he died, worn out with disease and toil, on March 3, 1703, and was buried in St. Helen's Church, Bishopsgate Street.

In personal appearance Hooke made but a poor show. His figure was crooked and his limbs shrunken; his hair hung in disheveled locks over his haggard countenance. His temper was irritable, his habits penurious and solitary. He was blameless in morals, and reverent in religion. His scientific performances would probably have been more striking if they had been less varied. He originated much, but perfected little. His optical investigations led him to adopt in an imperfect form the undulatory theory of light, to anticipate the doctrine of interference, and to observe independently of, though subsequently to, Grimaldi the phenomenon of diffraction. He was the first to state clearly

that the motion of heavenly bodies must be regarded as a mechanical problem, and he approached in a remarkable manner the discovery of universal gravitation. He suggested a method of meteorological forecasting and a system of telescopic signaling, anticipated Chladni's experiment of strewing a vibrating bell with flour, investigated the nature of sounds and the function of the air in respiration and combustion, and originated the idea of using the pendulum as a measure of gravity.[9]

His principal writings are *Micrographia or some Physiological Descriptions of Minute Bodies* (London, 1665, 1667),[10] *Lectiones Cutlerianae* (London, 1679), and *Posthumous Works*, containing a sketch of his "Philosophical Algebra," published by Richard Waller in 1705. A biography of Hooke was prefixed by Waller to the *Posthumous Works of Robert Hooke.*[11] This prefixture, entitled "The Life of Dr. Robert Hooke,"[12] which, by the way, has been referred to by Brewster[13] as if it were a separate publication and which utilized the beginning of an autobiography left by Hooke, gave, in Waller's words, "An Account of his Studies and Employments, with an Enumeration of the many Experiments, Instruments, Contrivances and Inventions, by him made and produced as Curator of Experiments to the *Royal Society.*" This biography, together with that in the ninth edition of the *Encyclopaedia Britannica,* has been used in what precedes.

Hooke was a very ingenious man and a skilful experimenter. Here we are only concerned with his speculations

[9] Cf. also W. W. Rouse Ball, *A Short Account of the History of Mathematics,* 4th. ed., London, 1908, p. 315. Cf. also Rosenberger, *op. cit.* pp. 71-73.

[10] Rosenberger, *op. cit.,* p. 35.

[11] *The Posthumous Works of Robert Hooke,.. Containing his Cutlerian Lectures and other Discourses Read at the Meetings of the Illustrious Royal Society. In which....,* published by Richard Waller, London, 1705.

[12] *Ibid.,* pp. i-xxviii.

[13] *Memoirs of the Life, Writings, and Discoveries of Sir Isaac Newton,* Edinburgh, 1855, Vol. I, pp. 285, 286; 2d. ed., 1860, Vol. I, p. 249.

on gravitation and his relations with Newton on this point. The biographical details into which we have entered are very much to the point when we are considering Hooke's work on the theory of gravitation. Hooke's education, owing to his health and worldly circumstances, was incomplete. Owing to these reasons and his ill fortune with his inventions and writings, he was jealous and embittered. His speculations show that he was, I think, quite the equal of Newton in imagination. As a mathematician he was vastly inferior to Newton, and hence his merits have been less noticed than they deserve to be, since they lie in the shadow of the great theory which was almost completed by Newton with unrivaled mathematical talent. And lastly, Newton himself seems to have underestimated the value of Hooke's work and perhaps even its influence on himself. Both in his researches on the theory of gravitation and in his discovery of the method of fluxions, Newton showed at first an almost complete lack of interest in his rights of authorship, and later, when a question of whether others had plagiarized from him arose, he showed himself touchy, suspicious, ready to attribute meannesses to others, and ready to expend great labor in the carrying out of a mean action. An example of this last unenviable characteristic is given by the discovery, after Newton's death, of a first draft in Newton's own handwriting of an account—in Newton's favor—of the dispute between Newton and Leibniz and their supporters about the invention of the infinitesimal calculus. This document was intended to appear as, and was indeed printed as, the unbiassed report of a committee of independent judges appointed by the Royal Society.

Newton's amanuensis has left on record how Newton, with apparent carelessness, used to leave a box of guineas lying about in his rooms at Trinity College, Cambridge. The amanuensis thinks it possible that this was done to

test the honesty of the old woman who made his bed and cleaned his rooms. It is difficult not to be forced to think that we see some analogy between his scientific and his domestic methods. If we resist this temptation, we shall have to assume that in early life Newton was either not conscious of the importance for science of his discoveries or even did not esteem science itself very highly, but that in later life his sense of property grew so strong that it embraced even things for which he did not care. I do not think that it can be maintained that Newton, at any rate until he was old, did not really esteem science highly. He wrote[14] indeed to Hooke in 1678 that he "had for some years past been endeavoring to bend" himself "from philosophy to other studies in so much," said he, "that I have long grudged the time spent in that study unless it be perhaps at idle hours sometimes for a diversion," and[15] that his "affection for philosophy" was "worn out, so that," as he said, "I am almost as little concerned about it as one tradesman uses to be about another man's trade or a country man about learning"; and[16] to Halley in 1686 that "Philosophy is such an impertinently litigious Lady that a man had as good be engaged in lawsuits as have to do with her. I found it so formerly, and now I am no sooner come near her again, but she gives me warning." Still, Newton fortunately did not then abandon "philosophy." He hated and was irritated by controversy, but in spite of this he only abandoned the science which gave rise to the controversies he suffered so impatiently when he was honored and prosperous and when nobody in England had the presumption to set up an opinion against that of the great and god-like Newton.

Nor could Newton have been under the delusion that

[14] W. W. Rouse Ball, *An Essay on Newton's Principia*, London, 1893, p. 141; cf. pp. 155, 157.

[15] *Ibid.*, p. 44.

[16] *Ibid.*, pp. 158-159.

his discoveries were of little moment to science. So we must, I think, infer that his character changed from the frankness shown in his first printed researches on optics[17] to the secretiveness and grudgingness to others shown in the manner in which the *Principia* was written, and in the jealousy, lack of candor and suspicion shown to all but his most intimate friends. This change was brought about, it seems, by the partly irritating controversies—the criticisms and objections of Hooke, Huygens, Pardies, Linus, Gascoigne, Lucas[18]—to which his early optical researches gave rise.

We will now turn to Hooke's publications. It seems true that, as Rosenberger[19] says: "Since Hooke, like Borelli, was a very clever physicist but no mathematician, he too could only be the percursor of a greater discoverer in whose brightness the lesser sun of his fame faded from view more than it should."

II.

As an indirect result of an inquiry into the nature of gravity instituted by the Royal Society in 1661,[20] Hooke on March 21, 1666, according to Birch,[21] "presented a paper, which was read, containing some experiments of gravity made in a deep well near Banstead Downs in Surrey; to which was annexed the scheme of an instrument for finding the difference of the weight, if any, between a body placed on the surface of the earth, or at a considerable

[17] Cf. Rosenberger, *op. cit.*, pp. 67-68.

[18] *Ibid.*, pp. 73-117.

[19] *Ibid.*, pp. 150-151.

[20] *Ibid.*, p. 151.

[21] Thomas Birch: *The History of the Royal Society of London for improving of Natural Knowledge, from its first rise. In which the most considerable of those Papers communicated to the Society which have hitherto not been published, are inserted in their proper order, as a Supplement to the Philosophical Transactions*, Vol. II, London, 1756, pp. 69-72; the extract is from pp. 69-70. Cf. pp. 77-78.

distance from it, either upwards or downwards. It was ordered that this paper should be registered[22] as follows:

" 'Gravity, though it seems to be one of the most universal active principles in the world, and consequently ought to be the most considerable, yet has it had the ill fate to have been always, till of late, esteemed otherwise, even to slighting and neglect. But the inquisitiveness of this later age has begun to find sufficient arguments to entertain other thoughts of it. Gilbert began to imagine it a magnetical attractive power inherent in the parts of the terrestrial globe; the noble Verulam [Francis Bacon] also, in part, embraced this opinion; and Kepler (not without good reason) makes it a property inherent in all celestial bodies,—sun, stars, planets. This supposition we may afterwards more particularly examine; but first it will be requisite to consider whether this gravitating or attracting power be inherent in the parts of the earth, and, if so, whether it be magnetical, electrical, or of some other nature distinct from either.

" 'First then, if it be magnetical, any body attracted by it ought to gravitate more, when nearer to its surface, than when farther off.' "

The ingenious experiments, which were conducted at the tops of Westminster Abbey and St. Paul's Cathedral, led, however, to no result.[23]

III.

On May 23, 1666, according to the Journal Book of the Royal Society,[24] there was read "a paper of Mr. Hooke's concerning the inflection of a direct motion into a curve by a supervening attractive principle," which was ordered

[22] Register, Vol. III, p. 93.

[23] Rosenberger, *op. cit.*, pp. 151-152; Mach, *Mechanics*, p. 532.

[24] Waller, *loc. cit.*, p. xii; Birch, *op. cit.*, pp. 90-92; Rouse Ball, *Essay*, p. 151.

to be registered.[25] "The discourse contained therein," says Waller, quoting Hooke, "is an introduction to an experiment to show that circular motion is compounded of an endeavor by a direct motion by the tangent and of another endeavor tending to the center."

This paper is as follows:[26]

"I have often wondered why the planets should move about the sun according to Copernicus's supposition, being not included in any solid orbs (which the ancients possibly for this reason might embrace) nor tied to it, as their center, by any visible strings; and neither depart from it beyond such a degree, nor yet move in a straight line as all bodies that have but one single impulse ought to do. For a solid body, moved in a fluid towards any part—unless it be protruded aside by some near impulse, or be impeded in that motion by some other obviating body, or that the medium through which it is moved be supposed not equally penetrable every way—must persevere in its motion in a right line, and neither deflect this way nor that way from it. But all celestial bodies, being regular solid bodies, and moved in a fluid and yet moved in circular or elliptical lines and not straight, must have some other cause besides the first impressed impulse that must bend their motion into that curve. And for the performance of this effect I cannot imagine any other likely cause besides these two: The first may be from an unequal density of the medium through which the planetary body is to be moved. That is, if we suppose that part of the medium which is farthest from the center, or sun, to be more dense outward than that which is more near, it will follow that the direct motion will be always deflected inwards by the easiest yielding of the inward and the greater resistance of the outward part of that medium. This has some probabilities attend-

[25] Register, Vol. III, p. 114.

[26] Birch, *op. cit.*, pp. 90-91.

ing it: thus, that if the ether be somewhat of the nature of the air, it is rational that that part which is nearer the sun, the fountain of heat, should be most rarified, and consequently that those which are most remote should be most dense; but there are other improbabilities that attend this supposition which, being nothing to my present purpose, I shall omit.

" 'But the second cause of inflecting a direct motion into a curve may be from an attractive property of the body placed in the center whereby it continually endeavors to attract or draw it to itself. For if such a principle be supposed, all the phenomena of the planets seem possible to be explained by the common principle of mechanical motions, and possibly the prosecuting this speculation may give us a true hypothesis of their motion, and from some few observations their motions may be so far brought to a certainty that we may be able to calculate them to the greatest exactness and certainty that can be desired.' "

Hooke illustrated the motions of the planets about the sun by means of an experiment with a conical pendulum.[27] The force to the center, in this case, increases with the distance, which is not so with the attraction of the sun. The experiment was thus merely superficially imitative and not explanatory, and the most interesting thing was a second pendulum-experiment which imitated the motion of the moon. Hooke added a smaller ball which he made to swing round the first by means of a short string that was fastened to the wire; he then remarked "that neither the bigger ball which represented the earth nor the less which represented the moon were moved in so perfect a circle or ellipsis as that in which they would otherwise have moved if either of them had been suspended and moved singly,

[27] Cf. Waller, *loc. cit.*, p. xii; Birch, *op. cit.*, pp. 91-92; Brewster, *op. cit.*, Vol. I, pp. 283-286; 2d ed., pp. 247-249; Rosenberger, *op. cit.*, pp. 152-153; S. P. Rigaud, *Historical Essay on the first Publication of Sir Isaac Newton's Principia*, Oxford, 1838, pp. 38-40.

but that a certain point, which seemed to be the center of gravity of these two bodies, however posited[28] (considered as one), seemed to be regularly moved in such a circle or ellipsis, the two balls having other peculiar motions in small epicycles about the same point."

"This last observation reminds us," says Rigaud, "of the fundamental property of motion in the eleventh section of the first book of the *Principia*; but that was much more than Hooke could have demonstrated, and indeed his experiment, though it answered the immediate purpose of illustrating his hypothesis, admitted no further reasoning to be derived from it as to the motion of the planets. He was aware that it did not represent the true action of gravitation, because the tendency towards the middle (there was no fixed point to which the body was constantly drawn) increased with the horizontal distance, which he knew to be wrong, although he gave no indication at the time of his knowing what the law really was by which the variation of gravity was regulated."

The eleventh section of the first book of the *Principia* is on the motions of bodies under their mutual attractions, and here, for the first time in the *Principia,* is applied the third law of motion—or rather that (fourth) corollary from it known as the principle of the conservation of the center of gravity. Hooke's name is not mentioned in connection with the third law in Newton's scholium, but here and, as we shall see, elsewhere, Hooke made quite perceptible advances towards both this law and the Newtonian conception of "mass." In neither case did he bring his thoughts into the form of a definite enunciation, but he may, I think, be considered as occupying in this respect a position intermediate between Descartes and Newton.

The instinctive nature of the third law may also be illus-

[28] Brewster has here the misprint "pointed."

trated by an extract from Birch's *History*[29] which immediately follows the account of Hooke's paper just referred to:

"Mr. Oldenburg produced a letter written to him by Dr. Wallis from Oxford, May 19, 1666,[30] in answer to the objections made at the preceding meeting against his hypothesis of the tides. This letter giving occasion to renew the discourse upon that subject, Dr. Goddard offered to the consideration of the Society this doubt, viz., supposing the earth and moon to move about a compound center of gravity, if the highest tides be at new moon when the earth is farthest from, and the moon nearest to the sun, and the tides abate as the earth approaches nearer till she comes into the supposed circle of her annual motion; why they do not abate, as the earth comes still nearer to the sun within the said circle? And so why we have not one spring-tide and one neap-tide in every course of the moon?"

Let us return to Hooke's idea—an idea which is a consequence of Galileo's considerations and was shared by Wren and Halley[31]—of compounding the planetary motions of a direct tangential motion and an attractive motion towards the central body. Hooke in a letter to Newton of 1679[32] asked for Newton's thoughts on this idea. Newton in his answer said[33] that so far as he remembered he had not before heard of this "hypothesis." Hooke, judging from his manuscript comments on this letter,[34] did not believe this statement. There seems no reason to doubt Newton's word; we know that such ideas were not peculiar to Hooke alone, and further, building on the dynamics of

[29] Vol. II, p. 93.

[30] Letter-Book, Vol. I, p. 320: It is printed in the *Phil. Trans.*, No. 16, pp. 281-283, end of the 2d section.

[31] Rouse Ball, *Essay*, p. 162.

[32] *Ibid.*, p. 140.

[33] *Ibid.*, p. 141.

[34] *Ibid.*, pp. 144-145, 152.

Galileo, Newton was surely quite capable of extricating and applying such an idea.

IV.

In March, 1671,[35] Hooke "showed several experiments to explain the nature and cause of gravity: particularly, on the 9th, an experiment was made in which some flour put into a void shallow glass with a large sloping brim and a pretty tall foot was made to rise and run over like a fluid, by the knocking on the edge of the glass, and also by the forcibly moving of one's finger round the edge of the same. Leaden bullets also being put into this glass, did, by knocking, move it like a fluid.... This was proposed to consider what might be the cause of gravity and suggest an hypothesis to explicate the motion of gravity by...."

V.

Later Hooke resumed the consideration of the subject of the planetary motions, and in a work which appeared in 1674[36] he published some interesting observations on gravity. "I shall hereafter," he says,[37] "explain a system of the world, differing in many particulars from any yet known but answering in all things to the common rules of mechanical motions. This depends upon three suppositions. First, that all celestial bodies whatsoever have an attraction or gravitating power towards their own centers whereby they attract not only their own parts and keep them from flying from them, as we may observe the earth to do, but that they also do attract all the other celestial bodies that are within the sphere of their activity; and consequently that not only the sun and moon have an in-

[35] Waller, *loc. cit.*, pp. xiv-xv.

[36] *An attempt to prove the Annual Motion of the Earth, from Observations made by Robert Hooke*, London, 1674; Cf.. *Phil. Trans.*, No. 101, p. 12. Cf. also Brewster, *op. cit.*, Vol. I, pp. 286-288, 2d ed., Vol. I, pp. 249-251; Rosenberger, *op. cit.*, pp. 153-155; Rouse Ball, *Essay*, pp. 151-152.

[37] At the end, pp. 27-28.

fluence upon the body and motion of the earth, and the earth upon them, but that Mercury and also Venus, Mars, Jupiter, and Saturn, by their attractive powers, have a considerable influence upon its motion, as in the same manner the corresponding attractive power of the earth has a considerable influence upon every one of their motions also. The second supposition is this, that all bodies whatsoever that are put into a direct and simple motion will so continue to move forward in a straight line till they are, by some other effectual powers, deflected and bent into a motion describing a circle, ellipsis, or some other more compounded curve line. The third supposition is that these attractive powers are so much the more powerful in operating, by how much the nearer the body wrought upon is to their own centers. Now what these several degrees are I have not yet experimentally verified; but it is a notion which, if fully prosecuted as it ought to be, will mightily assist the astronomer to reduce all the celestial motions to a certain rule, which I doubt will never be done true without it. He that understands the nature of the circular pendulum and of circular motion will easily understand the whole ground of this principle, and will know where to find direction in nature for the true stating thereof. This I only hint at present to such as have ability and opportunity of prosecuting this inquiry and are not wanting of industry for observing and calculating, wishing heartily such may be found, having myself many other things in hand which I would first complete, and therefore cannot so well attend it. But this I durst promise the undertaker, that he will find all the great motions of the world to be influenced by this principle, and that the true understanding thereof will be the true perfection of astronomy."[38]

[38] In quoting this passage, which Delambre (*Astronomie du 18me Siècle*, pp. 9, 10) admits to be very curious, he scarcely does justice to Hooke when he says that what it contains is found expressly in Kepler. It is quite true that Kepler mentioned as probable the law of squares of the distances, but he afterwards, as Delambre admits, rejected it for that of the simple dis-

"In this remarkable passage," says Brewster, "the doctrine of universal gravitation and the general law of the planetary motions are clearly laid down. The diminution of gravity as the square of the distance is alone wanting to complete the basis of the Newtonian philosophy, but even this desideratum was in the course of a few years supplied by Dr. Hooke." We will speak of this in the seventh section.

VI.

In Hooke's *Cometa* of 1678, he speaks not only distinctly of a "kind of gravitation by which the planets are attracted and have a tendency towards the sun as terrestrial bodies have towards the center of the earth,"[39] but considers comets as probably acted upon by the same force and moving by the effect of it in curvilinear paths.[40]

VII.

Hooke, after Oldenburg's death, became secretary of the Royal Society, and accordingly wrote a friendly letter on November 24, 1679,[41] to Newton, expressing a hope that he would continue to make communications to the Society, and, among other things, asking Newton to communicate to him any objection he might have to Hooke's idea of compounding tangential and central motions, referred to in the third section above. Newton, in his reply of November 28, excused himself from the proposed correspondence on the grounds of a cessation of his interest in "philosophy," but he courteously discussed some of the

tances. Hooke, on the contrary, announces it as a truth. Clairaut has justly remarked that the example of Hooke and Kepler shows how great is the difference between a truth conjectured or asserted and a truth demonstrated. This note is Brewster's.

[39] P. 31.

[40] P. 44. Cf. Rigaud, *op. cit.*, p. 38, and Rouse Ball, *Essay*, p. 157.

[41] Most of the letters, or drafts of them, together with Hooke's comments, in this correspondence between Hooke and Newton are in the Library of Trinity College, Cambridge, and are printed in Rouse Ball's *Essay*, pp. 138-153; cf. pp. 18-24. Cf. also Rosenberger, *op. cit.*, pp. 155, 167-171.

topics suggested by Hooke, and mentioned that it had occurred to him that a demonstration of the earth's diurnal rotation might be obtained by seeing whether a stone. when falling freely, deviated in an easterly direction from the perpendicular, and added that the free path was part of a spiral which passed through the earth's center. The question simply concerned the composition of the motion of the stone round the earth's axis with its falling motion.[42]

This letter was read to the Society on December 4, 1679; and to this reading refer Waller's[43] words:

"An experiment being suggested to try whether the earth moved with a diurnal motion or not, by the fall of a body from a considerable height, alleging it would fall to the east of true perpendicular, Mr. Hooke read a discourse upon that subject, wherein he explained what the line described by a falling body must be, supposing it to be moved circularly by the diurnal motion of the earth, and perpendicularly by the power of gravity, and showed it would not be a spiral line, but an excentrical-elliptoid, supposing no resistance in the medium, but supposing a resistance, it would be an excentric-ellipti-spiral, which after many revolutions, would rest in the center at last; that the fall of the body would not be directly east but to the south-east, and more to the south than the east. This was tried, in which the ball was still found to fall to the south-east."[44]

On December 9, Hooke wrote again to Newton;[45] the letter is lost, but it seems to have been to the same effect as the communication to the Royal Society. Newton's reply is missing, but Newton wrote to Halley in 1686 that Hooke's "letters occasioned my finding the method of determining figures" described under a central force and

[42] Birch, *History,* Vol. III, 1757, pp. 512-513; Rouse Ball, *Essay,* p. 145.

[43] *Loc. cit.,* p. xxii.

[44] Cf. Birch, *History,* Vol. III, p. 516; Rouse Ball, *Essay,* p. 146.

[45] Rouse Ball, *Essay,* pp. 145, 152.

finding thus the ellipse,[46] and "his correcting my spiral occasioned my finding the theorem by which I afterwards examined the ellipsis."[47]

It must be added that in Hooke's letter of January 6, 1680,[48] the law of attraction (varying as the inverse square of the distance) is explicitly stated,[49] and Newton, in the scholium to the fourth proposition of the second section of the first book of the *Principia,* gave Hooke credit for this.

These are the facts on which was based the controversy between Hooke and Newton about the discovery of the theory of gravitation. We shall here pass over this controversy, and in the next section will give some account of Hooke's fundamental physical views, among which is the beginning of a conception of "mass" as distinct from the Cartesian idea of a plenum of matter whose essence is extension. Hooke considered an ether containing pieces of matter, apparently qualitatively different.

However, we must here give some account of a rather pathetic incident which occurred as a result of the controversy between Hooke and Newton about the discovery of the theory of gravitation.

In 1813, says Rigaud,* a collection of letters was printed from the originals in the Bodleian Library of Oxford, with some biographical notices taken from the manuscripts of J. Aubrey in the Ashmolean Museum of Oxford. Among these last was a letter from Aubrey to A. Wood on the claims of Hooke to the discoveries which were said to pass for Newton's. Aubrey's own authority on such a point is not great, and the letter therefore, as it is printed,

[46] *Ibid.,* p. 165.

[47] *Ibid.,* p. 167.

[48] It must be remembered that at that time in England the year began on March 25, so that this date was then January 6, 1679.

[49] Rouse Ball, Essay, p. 147.

* *Op. cit.,* pp. 41-42.

is not only obscure, but deprived of its real value. The editor has given a long passage which he states to be taken from what is in Hooke's handwriting; but he has inserted it in the wrong place, in such a manner as to make it appear to be merely a loose paper which Aubrey had sent to his correspondent. By a reference however to the the original, it was found that the letter must have been written under Hooke's immediate direction; it is not only corrected and altered by him in many places, but by far the greater part consists of additions which he has made with his own hand. This letter is reproduced by Rigaud.* As Rigaud's work is rather rare we will here reproduce the letter of Aubrey and Hooke spoken of.

"Sept. 15, 1689.

"Mr. Wood!

"Mr. Rob. Hooke, R.S.S., did in anno 1670 write a discourse called, An Attempt to prove the Motion of the Earth, which he then read to the Royal Society; but printed it in the beginning of the year 1674....† to Sir John Cutler, to whom it is dedicated, wherein he has delivered the theory of explaining the celestial motions mechanically; his words are these, pag. 27, 28. viz.‡

"About 9 or 10 years ago Mr. Hooke writ to Mr. Isaac Newton of Trin. Coll. Cambridge, to make a demonstration of [it] *this Theory, not* telling him *at first* the proportion of the gravity to the distance, [and] *nor what was* the curved line that was thereby made.

"Mr. Newton [did express], in his answer to the letter, *did* express that he had not thought§ of it; and in his first

* *Ibid.*, pp. 52-55 of Appendices. In the following letter, whatever was inserted by Hooke is in italics. Aubrey's words are enclosed in square brackets when they have been erased in order that others might be substituted for them.

† The words here are not legible, but probably they make mention of Hooke "as lecturer to Sir John Cutler," or something to that purport.

‡ Here a space is left in which Aubrey evidently intended to insert the passage.

§ "known" is written over "thought"; but the first word is not erased.

attempt about it, he calculated the curve by supposing the attraction to be the same at all distances: upon which Mr. Hooke told him in his next letter the whole of his Hypothesis, scil. that the gravitation was reciprocal to the square of the distance, *which would make the motion in an ellipsis, in one of whose foci the sun being placed, the aphelion and perihelion of the planet would be opposite to each other in the same line, which is the whole celestial theory, concerning which Mr. Newton hath made a demonstration,* not at all owning he received the first intimation of it from Mr. Hooke. Likewise Mr. Newton has in the same book printed some other theories and experiments of Mr. Hooke's *as that* about the oval figure of the earth and sea, without acknowledging from whom he had [it] *them, though he had not sent it up with the other parts of his book, till near a month after this theory was read to the Society by R. H.,* (Mr. Hooke,) *when it served to help to answer Dr. Wallis his arguments produced in the R. S. against it.*

"*In the Attempt to prove the Motion of the Earth, etc., printed 1674, but read to the Royal Society 1671, pag. 27, lin. 31.*

" '*I shall only for the present hint, that I have, in some of my foregoing observations, discovered some new motions even in the earth itself, which perhaps were not dreamt of before, which I shall hereafter more at large describe, when further trials have more fully confirmed and completed these beginnings. .At which time also I shall explain a system of the world.** *But these degrees and proportions of the power of attraction in the celestial bodies and motions were communicated to Mr. Newton by R. Hooke in the year 1678 by letters, as will plainly appear both by the copies of the said letters, and the letters of Mr. Newton in answer to them, which are both in the custody*

* Here follows the extract already reproduced in § V above down to "experimentally verified."

*of the said R. H., both which also were read before the Royal Society at their public meeting, as appears by the Journal Book of the said Society.'**

"Mr. Wood!

"This is the greatest discovery in nature, that ever was since the world's creation: it never was so much as hinted by any man before. I know you will do him right. I hope you may read his hand: I wish he had writ plainer, and afforded a little more paper.

Tuus,

"J. AUBREY....

"Before I leave this town I will get of him a catalogue of what he hath wrote, and as much of his inventions as I can; but they are many hundreds; he believes not fewer than a thousand. 'Tis such a hard matter to get people to do themselves right."

Other letters† in the Bodleian, of which Rigaud's extracts are given here, show that Hooke had been incessantly urging Aubrey to procure some notice of him by Wood. Thus:

"London, Sept. 15, 1674. Mr. Hooke told me, (who has looked over your book‡), that you have left out several eminent men. You have not either mentioned him, which I desired. England has hardly produced a greater wit, viz. for mechanics."

"Gresham Coll., March 2, 1691-2. Mr. Wood! I acquainted you, some weeks since, that Mr. Hooke (now Dr. Hooke) desired you to do him the favor to send him a transcript of what you are to print concerning him. I have not yet heard from you about it: and Dr. Hooke doth again

* In the Journal Book of the Royal Society there is no mention of any such correspondence in 1678, or in 1679 till December. It was a mistake therefore in Hooke to refer to the former of these two years.

From this point the quotation, *"But it is a notion....the true perfection of astronomy,"* is resumed.

† *Ibid.*, pp. 56-57 of Appendices.

‡ *Historia et Antiquitates Univers. Oxon.*; published in 1674.

this day earnestly desire you would be pleased to write as aforesaid, as soon as you can possibly: for it doth (he says) exceedingly concern him. He will repay you for the transcription, which I shall deliver to you when I come to you."

"London, March 3, 1691-2. Mr. Wood! I sent you a letter some weeks since that Dr. Hooke remembers him very kindly to you, and does earnestly request you to do him the favour to send him a transcript of what you intend to write of him, with all possible speed; and he will repay you for the transcribing. To this purpose I yesterday left a letter with Mr. Bennet; but to-day speaking with Mr. Bennet, he tells me that he sent a letter from you to me, by the penny-post, on Saturday last: my landlady affirms she received it not. Now your book* drawing to an end, I, not knowing what the consequence of that letter may be, thought it a sure way to trouble [you] with this letter by the post."

"April 13, 1692, Gresham College. Dr. Hooke does again desire, if you do make any mention concerning him, you would favour him with a copy of it, before it goes to the press, and he will gratify you in anything that is equivalent. He remembers him kindly to you, and will be ready to serve you in any thing that may lie in his way."

VIII.

To "A Discourse of the Nature of Comets; read at the Meetings of the Royal Society soon after *Michaelmas*, 1682," in Hooke's *Posthumous Works*,[50] "we have.... here annexed," says Waller,[51] "a pretty large, and (if I may be allowed to speak) an ingenious discourse of gravity or gravitation. The running title will direct the reader to it. In this he considers the most known properties of

* *Athenae Oxonienses*, of which the first edition was completed and published in 1692.

[50] *Posthumous Works*, pp. 149-185; cf. Rosenberger, *op. cit.*, pp. 156-157.

[51] *Posthumous Works*, pp. 149-150.

the celestial bodies, and having made several deductions from observations, as to the nature of the ether, air, and the like, in which *in transitu* he explains thunder and lightning, asserting a levitation, as well as gravitation (or a receding from, as well as tendency towards the center); having also shown that the ether or vast fluid *expansum* is the medium to convey the motions of gravitation as well as light; he comes in the next place to treat more particularly of body and motion, explaining what he understands by each of them, and then treating of motion, says that the two great laws of motion are light and gravity, and, having before treated of the former, he comes to explain the latter more particularly when, having shown that there is such a thing as gravity, with the limits and proportions of its power, and that it exerts it in all bodies, he comes at last to the principal part, the cause of gravity, and after the enumeration of its properties, gives his explication and hypothesis of the cause thereof. The author designed to have answered several objections against this his hypothesis, but having replied to one only, the discourse ends. To supply this defect, I have added some fragments which I found relating to the same subject, which the reader will find immediately annexed."

"All solid celestial bodies, then," says Hooke,[52] "have two properties; first a faculty of emitting or reflecting light; secondly an orbicular figure.... The second property of their orbicular or spherical form is an indication of another active principle which I conceive universal to all solid bodies in nature, and that is, of a gravitation or power of attracting similar solid bodies towards their centers. Which two principles I take to be the most considerable and the most active in nature and those from which the most considerable effects are produced; and when they are understood and explained as they ought, I

[52] *Ibid.*, p. 166.

question not but that they will afford us solutions and reasons for a thousand phenomena, the explication of which does now so much puzzle and perplex us."

From his observation he deduced,[53] among other things, that "the power of gravitation is extended into the ether, without the atmosphere of bodies, and consequently that the atmosphere or air is not the cause of gravitation, but rather the ether, in which the atmosphere or air is but a kind of dissolution, as salt or tinctures are dissolved in water or other liquors, and that from thence comes even the gravitation of the atmospheres to their incompassed bodies; which we observe by many other experiments made here upon the earth. For it is evident that bodies in a receiver, exhausted or emptied of the air by means of the exhausting engine, or any other ways, have not less of gravity towards the perpendicular or center of the earth than the bodies in the open and free air; nay they are found to be proportionably heavier, by how much a body of the air, equal to them in bulk, has been found to be lighter than them; which is an experiment that has often been tried."

To prove these deductions, Hooke demonstrated "four particulars"; first explaining what notions his expressions are meant to convey.[54]

"I conceive then the whole of realities that any ways affect our senses to be body and motion. By body I conceive nothing else but a reality that has extension every way, positive and immutable, not as to figure but as to quantity; and that the body, as body, is the same whatever figure it be of, as a quart of water is a quart of water, or a certain quantity of body, though contained in a globe, cylinder, cone, cube, quart pot, or any other figured containing vessel: and as body, it is indifferent to receive any figure whatever; nor has it more extension in one than in

[53] *Ibid.*, pp. 168-169.
[54] *Ibid.*, pp. 171-172.

the other vessel, nor can it have less; nor is it more essentially a body when solid, as ice, than when fluid; that is, the minims of it are equally disposed to motion or rest in position to each other; and therefore body, as body, may as well be, or be supposed to be indefinitely fluid as definitely solid; and consequently there is no necessity to suppose atoms, or any determinate part of body perfectly solid, or such whose parts are incapable of changing position one to another; since, as I conceive, the essence of a body is only determinate extension, or a power of being unalterably of such a quantity, and not a power of being and continuing of a determinate quantity and a determinate figure, which the anatomists suppose. These I conceive the two powers or principles of the world, to wit, body and motion; uniformity of motion making a solid and difformity of the motion of the parts making a fluid, as I shall prove more at large by and by.

"By motion I understand nothing but an alteration, or power of alteration of the minims of a whole in respect of one another, which power may be increased or diminished in any assignable quantity; but the natural balance of the universe is reciprocal to the bulk or extension, or to the quantity of the other power, body.

"These two I take to be two single powers, which cooperate in effecting the most of the sensible and insensible effects of the world."

And again:[55]

"As for matter, that I conceive in its essence to be immutable, and its essence being expatiation determinate, it cannot be altered in its quantity either by condensation or rarefaction; that is, there cannot be more or less of that power or reality, whatever it be, within the same expatiation or content; but every equal expatiation contains, is filled, or is an equal quantity of *materia*; and the

[55] *Ibid.*, pp. 172-173.

densest or heaviest, or most powerful body in the world contains no more *materia* than that which we conceive to be the rarest, thinnest, lightest, or least powerful body of all; as gold, for instance, and ether, or the substance that fills the cavity of an exhausted vessel, or the cavity of the glass of a barometer above the quicksilver. Nay, as I shall afterwards prove, this cavity is more full, or a more dense body of ether, in the common sense or acceptation of the word, than the gold is of gold, bulk for bulk; and that because the one, viz., the mass of ether, is all ether; but the mass of gold which we conceive is not all gold but there is an intermixture, and that vastly more than is commonly supposed, of ether with it; so that the vacuity, as it is commonly thought, or erroneously supposed, is a more dense body than the gold as gold. But if we consider the quantity of the whole content of the one with that of the other, within the same or equal quantity of expatiation, then are they both equally containing the *materia* or body.

"This possibly may at first hearing seem a little paradoxical, if not absurd; however I doubt not but that by the sequel of my discourse I shall be able to make it somewhat more plausible, if not positively and undeniably demonstrate it so to be.

"The second principle or power, which is motion, is of a quite differing nature, and may be rarified and condensed, diminished or increased, within the same quantity of body or matter, in any proportion assigned; that is, the same quantity of the first power, body or part of matter, may receive any assignable quantity of the second, that is, any assignable degree of motion; and being possessed of it, it may communicate or lose any assignable part of what it has, and still the body, as body, remain unaltered and the same; for as it may be moved with any motion, how swift soever it be supposed; so may it move with indefinitely slow motions, and that so far as that the next step one

would suppose it must lose all its motion, and remain in entire rest, and unalterable of position, as to the contiguous body."

With regard to the second of the "great laws of motion which constitute the form and order of the world"—gravity, the first being light, Hooke[56] says:

"By gravity then I understand such a power as causes bodies of a similar or homogeneous nature to be moved one towards the other till they are united; or such a power as always impels or drives, attracts or impresses motion into them, that tends that way, or makes them unite. The universality of this principle throughout the whole and everything therein I shall afterwards have more occasion to explain when I come to the effects of nature in the lesser bodies. At present I shall only proceed to show it in the greater bodies of the world."

"There have been," says Hooke,[57] "as many differing opinions concerning the limits of this power: some extending it too far, and others as extravagantly too little; some supposing that wheresoever in the universe a terrestrial body should be placed, there it would have a tendency towards the center of the world or earth; and therefore that in the creation all the terrestrial matter of the chaos met together and made up the body of the earth. Others, on the other hand, have been too penurious in limiting its power to some few miles; some to fifty miles, others to a boundary that a cannon well charged with powder would be able to shoot a bullet out of its reach. But though they are both enough mistaken, yet they agree in this, that this power of gravitation does act at some distance above the surface of the earth."

On the next page, Hooke[58] asserts:

[56] *Ibid.*, p. 176.
[57] *Ibid.*, p. 177.
[58] *Ibid.*, p. 178.

"I say, moreover, that this power is not only placed in the earth, but that there is the like power in every globular body in the universe, whether sun or fixed star, planet primary or secondary, and in the cometical body included, as I have supposed, within the nucleus of white cloud appearing in the head."

There are many other intelligent remarks on gravity and the explanation by its means of celestial motions. But here our object is to collect what details we can of the growth and the conception of mass and its connection with "the quantity of matter."

The bodies most receptive of gravity, according to Hooke,[59] "are such as have their particles of the greatest bulk and of the closest texture. This the whole series of grave bodies will sufficiently manifest; and I shall afterwards prove, when I come to show the texture of body, what it is that causes bodies to be grave or heavy and what makes them light, and that it is not the quantity of matter contained within the same space, but the modification of that matter, and the receptivity it hath of uniform power."

"I cannot find by any certain experiment," says Hooke,[60] "that grave bodies do sensibly decrease in gravity, though further removed from the surface of the earth; which was the intent to an experiment I formerly tried at the top of the steeple of St. Paul's and at Westminster Abbey, and may now again be repeated with much more conveniency and greater advantage at the column on Fishstreet Hill. For by counterpoising two weights in a curious pair of scales, first at the top of the steeple and then letting down one of the weights by a wire of two hundred and four feet in length, the counterpoise remaining at the top in the scale, the *aequipondium* remained: whereas if the gravity

[59] *Ibid.*, p. 182.

[60] *Ibid.*, pp. 182-183

of the body had increased by approximation to the earth, the weight let down to the bottom must have weighed the heavier. But though the difference were insensible in so small an height, yet I am apt to think some difference may be discovered in greater heights, and in some more curious ways than those I then used, even in that height; for I shall in my following discourses plainly show from the theory thereof that there is necessarily a difference and that the power of gravity does decrease at farther and farther distance from the center of the earth, and consequently that the line of a projected descending body is not truly parabolical, but elliptical though it should be made *in vacuo,* where the impediment of the medium could make very little or no alteration."

Then he deals[61] with his hypothesis as to the cause of gravity:

"Suppose then that there is in the ball of the earth such a motion, as I, for distinction's sake, will call a globular motion, whereby all the parts thereof have a vibration towards and fromwards the center, or of expansion and contraction; and that this vibrative motion is very short and very quick, as it is in all very hard and very compact bodies: that this vibrative motion communicates or produces a motion in a certain part of the ether which is interspersed between these solid vibrating parts: which communicated motion causes this interspersed fluid to vibrate every way *in orbem,* from and towards the center, in lines radiating from the same. By which radiating vibration of this exceeding fluid and yet exceeding dense matter, not only all the parts of the earth are carried or forced down towards the center; but the motion, being continued into the ether interspersed between the air and other kinds of fluid, causes those also to have a tendency towards the center; and much more any sensible body whatever that is

[61] *Ibid.,* pp. 184-185.

anywhere placed in the air or above it, though at a vast distance; which distance I shall afterwards determine and show with what proportioned power it acts upon bodies at all distances both without and within the earth: for this power propagated, as I shall then show, continually diminishes according as the orb of propagation continually increases, as we find the propagations of the media of light and sound also to do; as also the propagation of undulation upon the superficies of water. And from hence I conceive the power thereof to be always reciprocal to the area of superficies of the orb of propagation, that is duplicate of the distance; as will plainly follow and appear from the consideration of the nature thereof, and will hereafter be more plainly evinced by the effects it causes at such several distances.

"This propagated pulse I take to be the cause of the descent of bodies towards the earth. But it may perhaps seem a little strange how the propagation of a motion outward should be the cause of the motion of heavenly bodies downwards. To make this the more intelligible, I shall mention an observation very commonly known amongst tradesmen; and that is, the driving of a hammer or axe upon the helve, which to do the easiest way, they commonly strike the end of the helve, holding the helve in their hand and the axe or hammer at the lower end hanging downward, by which means they not only make the axe to go on upon the helve, but make it ascend, if they continue striking, even to their very hand. To apply which observation to my present theory, I say that the medium of propagation is the helve, and the axe or hammer is the grave body that descends; so that at every stroke that is given by the globe of the earth to the propagating medium, one degree of velocity of descent is given to the grave body which is as it were the axe. Now according to the velocity of this vibrative motion of the earth, so must the power it commu-

nicates be stronger or weaker. Suppose for instance, there should be a thousand of these pulses in a second of time, then must the grave body receive all those thousand impressions within the space of that second, and a thousand more the next, and another thousand the third second, so that in equal times it would receive equal degrees of acceleration. And if a second of time were again subdivided into a thousand moments of time, the body would receive one degree of acceleration in the first moment, one more in the second, a third in a third, and so onwards; so that the compounded acceleration would be as one in the first second, three the next second and five the next, and so onwards; according as it is observed in the motion of descending bodies.

"The medium that propagates this motion, I suppose to be one part of that which permeates most bodies, which we call by the general name of ether, and thence it proceeds that the motion is communicated to every part thereof: and so the momentum of every body becomes proportioned to its bulk or density of parts, difform to the fluid medium that communicates the pulse."

Then he[62] begins to discuss objections that might be urged against it, when the manuscript breaks off abruptly.

Among the loose papers of Hooke's which are printed after this is "An Account of Dr. Isaac Vossius's Hypothesis of Gravitation, with some animadversions thereupon."[63]

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[62] *Ibid.*, p. 185.
[63] *Ibid.*, pp. 201-202.